

Interactive comment on “Data limitations and potential of hourly and daily rainfall thresholds for shallow landslides” by Elena Leonarduzzi and Peter Molnar

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The study examines the performance of rainfall thresholds for landslides obtained from different hourly and daily datasets, as well as the use of normalizations for the threshold localization and some preliminary analyses on the impact of antecedent conditions.

The manuscript is well written, the study well fits the topics of this journal and is carried out with sound methods and data. To my view, the novelty brought by the study is that it collects from an amount of recent theoretical and smaller-scale studies and collectively examines the practical implications using a large dataset on a wide alpine region. I believe it contributes to our practical knowledge on rainfall thresholds for landslides

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triggering and therefore deserves publication. Overall, it was a pleasant reading.

I list below a few comments for the author's consideration.

Kind regards,

Francesco Marra

1. In Section 3.4, did you check the results using absolute quantiles (corresponding to return levels, or probabilities in time) instead to wet quantiles? To my view, the number of wet days contributes generating the local climatology (indeed it does for return levels) and the wet-quantiles somehow forget this. I refer in particular to the discussion in lines 281-291, which I believe would hold more for absolute quantiles. Also, it would be very interesting to see fig 6 with absolute quantiles (return levels) instead of wet-quantiles. I'm not saying this must be included, rather that it should be checked before exclusion (even though I'd be personally interested in seeing the figure anyways)

2. I suggest including ID/ED thresholds in the results in Fig. 3. Many readers are familiar with such thresholds and it would be helpful for the quantitative interpretation of the results

3. Do your archives contain information on the landslides type? Are debris flows included in the database? I would guess that debris flows, generally triggered by short convective events, are more subject to the temporal resolution. If relevant, is there a way to check this from your data? Also, in the discussion (lines 355-356) you recommend "not to extend daily thresholds . . . into the sub-daily domain" – can this recommendation be made more explicit from the elements in your hands?

4. The title focuses on the temporal resolution aspect while the paper provides quite a lot of additional information. Perhaps you can consider expanding it

5. Lines49-54: More details on the point (b) (i.e. poor matching of landslide in space) should be provided in the introduction as this is a crucial to the findings. There are few lines afterwards but I think some (more) lines are needed in the introduction as well

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6. Line 58: what do “(analysis steps)” refer to?
7. Line 97: could you provide more details on the optimization (what was optimized, how, why)?
8. Line 181-182: “Averaging...” this sentence was not clear to me. Also what do you mean by “trends”?
9. Line 195: it looks like RDI retains good predictive power because of the stations density, is this correct? Are there other reasons?
10. Line 215: perhaps I did not understand: why does the sparseness of the points in the figure imply lower robustness? Is this not just a consequence of the data sample?
11. Line 323-329: I’d argue that Marra 2019 do not claim/confirm that higher resolutions are superior as no evaluation of the predictive performance was done. Rather, systematic differences are highlighted, with consequences for the physical interpretation of the triggering amounts and the quantitative comparison of thresholds and threshold parameters obtained from different datasets
12. Fig. 2 and 3 took me some time to understand. I could not find suggestions on how to make them more immediately understandable, but I feel it is something to communicate within the review

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